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ABSTRACT

A decline of faith in the efficacy of education has been furthered by the perceived failure of social action programs to reduce poverty and assure racial justice and by the educational research purporting to show that the impact of schools is slight compared to other factors. One response open to educators is to consider policies other than those customarily employed by educators to affect education outcomes. Three such areas--policies associated with improving health and nutrition, eradicating income poverty, and improving housing or promoting economic or racial integration--and how they affect school performance are examined here. In each case the results are equivocal--the policies may have some effect on education outcomes, but the evidence is too flawed to rely on. Policy-makers generally choose one of three courses in using research in the formation of policy--"least risk," "best bet," and "winning line." "Winning line" is the correct approach. In this approach one has more-or-less well-defined goals and formulates policy by asking in what states of the world those goals can be achieved. One then proceeds as if those states of the world existed. (Author/IRT)

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Healthy, Wealthy, and Wise: Backdoor Approaches to Education

Henry J. Aaron

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION

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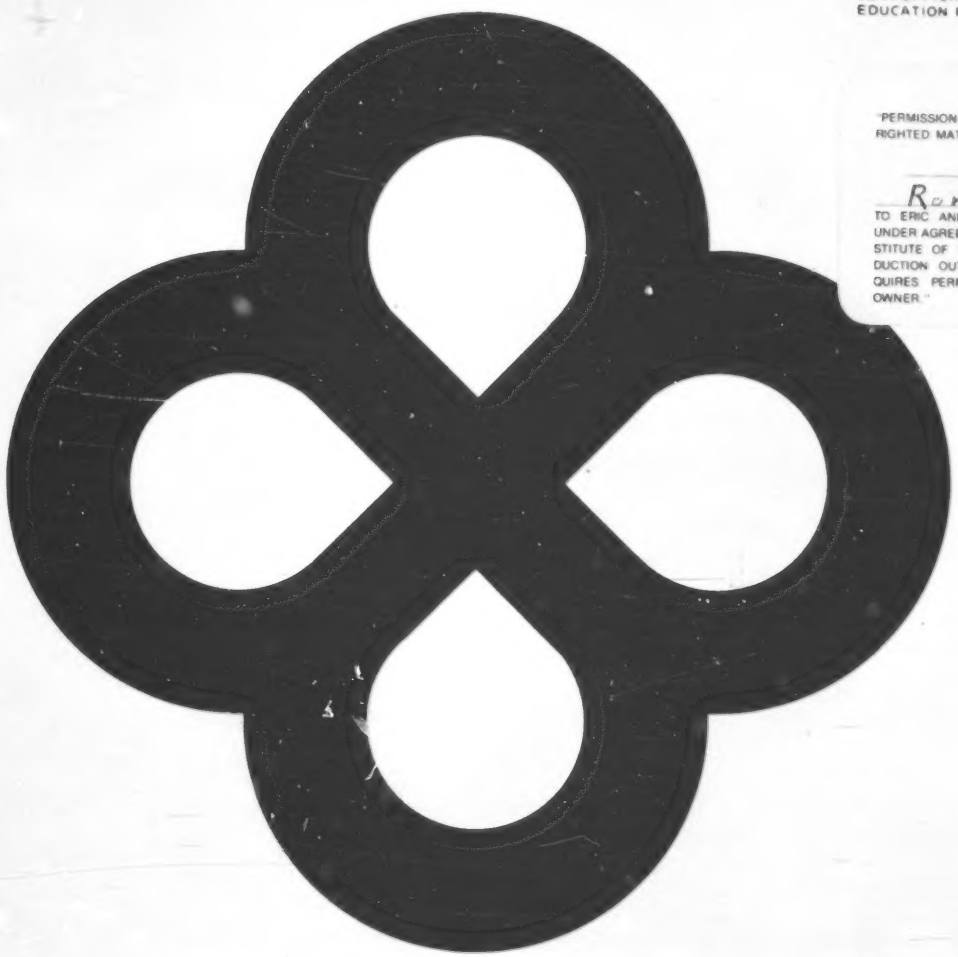

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HEALTHY, WEALTHY, AND WISE:

BACKDOOR APPROACHES TO EDUCATION

by Henry Aaron*

I

Within the last few years, popular faith in the power of education to improve society, to reduce income inequality, to break down social stratification—one might almost say, to do whatever the speaker feels it is important to accomplish—was severely shaken. The old euphoric faith was supplanted by a new, depressing skepticism that education had little impact on the life chances of students. According to this view, we had no idea how to alter education outcomes even if we benightedly adhered to the passé belief, known by the informed to be untenable, that education does affect life chances. The transformation in popular attitudes was as remarkable as it seemed complete. Now, however, such skepticism is waning among those who produce the increasing flow of research and writings on education, and popular interest in improving schools is once again strong, even if confidence in our ability to do so remains weak.[1]

Two related but separate sets of developments were associated with the decline of faith in the efficacy of education. First, the commitments of the Johnson administration to reduce poverty and to redress past injustices, particularly racial ones, were expressed in flamboyant rhetoric that accorded education a prominent place. Frustration over the perceived failure to achieve the objectives of the War on Poverty was transferred to the means adopted to achieve those goals. The efforts subsumed under the Great Society promised not only to provide cash or commodities to the poor, but also to help the poor transform themselves into self-supporting, tax-paying, job-holding, stable families. Not only would the Great Society put a floor under the economic position of the deprived, but it promised to help the poor build the support for themselves by increasing their "human capital" through investments in education and training.

This rhetoric was accepted by the public as an accurate description of what the Great Society was about, despite the fact that expenditures to improve education and training by the federal government were paltry in comparison with the increase in expenditures on cash support, subsidized health care, housing, and food, or in comparison with increases in expenditures on education by state and local governments. As a fraction of full employment GNP, expenditures on education and training by the federal government rose from .32 percent in 1961 to .73 percent in 1975, compared with an increase in expenditures on cash assistance and subsidized commodities from 7.8 percent to 11.3 percent. At the same time, rapid increases in state and local education expenditures continued, largely in response to the postwar baby boom still working its way through publicly supported elementary, secondary, and higher education, and partly to improve salaries

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and reduce classroom size. Expenditures on education by state and local governments continued their climb, from 2.5 percent of GNP in 1950 to 3.7 percent in 1960 and 5.4 percent in 1970.

The result of these outlays has been spectacular by some standards but disappointing by others. Completion rates in secondary school for whites rose from 69.3 percent in 1962 to 83.3 percent in 1974; for blacks, the rates went from 41.6 percent in 1962 to 68.3 percent in 1974 (U.S. Census Bureau.)[2] College enrollments have reached a plateau at a high level (U.S. National Center for Education Statistics, 1976, p. 91). In contrast, scores on standardized tests have begun to decline. Also, the salary advantage of new college graduates may have narrowed (Freeman, 1975).[3] This narrowing, a source of hope to egalitarians and of relief to economists who have been wondering why it didn't happen sooner, must depress those lower-income parents and their children who viewed a college degree as their ticket into the upper social and economic strata. More vaguely, but perhaps more significantly, voters may have become rather annoyed in the 1960s that large increases in education expenditures did not prevent college and high school students from behaving rather badly, as judged by the prevailing moral standards of middle-income households who make up the bulk of U.S. taxpayers (Wilson, 1968).[4] From the transformation of declared sexual morality through the growing acceptability of obscenity to what Banfield (1968) has labeled "rioting mainly for fun and profit," the sensibilities of the straight, staid middle class were violated by the young. Although the tide of effrontery has receded, support for education has not fully recovered.

For each of these reasons, what was popularly perceived as (and, at the local level, was) a major national commitment of resources has produced rather less than people were led to believe they should get for their money. Education did not eliminate poverty, produce a nation of well-mannered young adults, or break down the barriers to upper-class position in the ways people had come to expect.

The second set of developments associated with the decline in the perceived efficacy of education has been the flow of research addressed to (a) how education could be modified to improve performance, especially of disadvantaged students, as measured by standardized tests or other objective criteria, and (b) how education affects the income and social position of its recipients. This research helped change the perception of education by educators, the lay public, and analysts themselves. The dominant interest of the pre- and early 1960s in curriculum (e.g., the progressive movement, the changes in science and mathematics curricula) and in the adequacy of facilities *qua* inputs gave way to a concern about the relationship between education and outputs or results, such as income or socioeconomic status. This was the essence of the human capital revolution, embodied politically in the vision of the Great Society to be won through victory in the War on Poverty.

The dominant event in the history of such research is the report on *Equality of Educational Opportunity* (U.S. Department of Health, Education and Welfare, 1966)—the *Coleman Report*. Based upon statistical analysis of survey data collected to satisfy requirements of the Civil Rights Act of 1964, the *Report* detailed two major conclusions, one factual, one inferential. The factual conclusion—that school resources available to racial and ethnic groups differed negligibly in 1965—has gone largely unchallenged. The inferential conclusion—that differences among measurable characteristics of schools explain little of the variation in performance on standardized tests, that the socioeconomic status of peers and family background characteristics are far more important, but that most of the variation in school performance is unexplained—provoked a controversy so intense that it spawned a subspecialty in education research. The study seemed to imply that educators should pay less attention to the tangible aspects of education they could control, such

as class size, availability of science labs, and curriculum, and relatively more attention to the things they could not directly control, such as the socioeconomic characteristics of schools and family background characteristics of students, that they had formerly regarded as beyond their professional purview. A second implication was that most of the variation in performance was beyond the control of any of these influences, or at least beyond those that had been measured.

Rather paradoxically, the force of the *Coleman Report* was magnified by the impassioned and, in some cases, devastating criticisms to which it was subject (see Bowles and Levin, 1968, Cain and Watts, 1970, and Hanushek and Kain, 1972). The data on which the *Report* was based were so clearly superior in quantity, if not in quality, to any other then available that even damning criticisms of the absolute quality of the data and particularly of the analytical methods employed did more to focus attention on the *Report* and spread the influence of its findings than could possibly have occurred if it had been studiously ignored. Journal articles and books employing the data to examine questions neglected in the *Report* to reexamine other questions treated incorrectly all were forced into the procrustean bed formed by the categories and characteristics of the data collected (see the various essays in Mosteller and Moynihan, 1972). Like the drunk in the old vaudeville routine who looked for his lost wallet under the lamp post although he had lost it elsewhere, those who denied the acceptability of the *Report's* conclusions were forced to use data whose characteristics and shortcomings very nearly precluded interpretations other than those the *Report* had reached. The *Report* gained force also because other research could detect no consistent results from the various interventions educators and others expected or hoped would improve performance (see Averch *et al.*, 1975). The fact that these data could have been generated by a world that functioned quite differently from that alleged by the *Report* has only begun to emerge. Education and its quality as measured by objective inputs may significantly and powerfully affect income and occupational status, but these effects might be nearly invisible in the kind of data on which the *Coleman Report* is based (Luecke and McGinn, 1975).[5]

In general, cross sectional data of the kind used in the *Coleman Report* and much education research may conceal the effects of important influences on education outcomes (Hanushek and Kain, 1972). A simple example may make this possibility clear. Suppose that education outcomes as measured by tests depend on expenditures per pupil per year in all grades since kindergarten. If students move among schools with different spending levels, a simple cross section study of the relationship between spending per pupil (even if spending is well measured, as it was not in the *Coleman Report*) and educational performance will understate any relationship that may exist.

Furthermore, the statistical analysis in the *Coleman Report* is an attempt to discover how much of the variation in test scores of a large population of students at a single point in time is associated with variations in a wide range of observable characteristics. The data in many cases are merely averages of schools and school districts and do not reflect the actual experiences of pupils. The model on which the analysis is based is a simple one and does not allow for complex effects, for example, that high performance and ambition of the student body may help some students and hurt others.

Finally, estimates of the contribution of differences in education inputs to test scores are understated to the extent that education resources are used to remedy deficient performance. Since more education inputs than average are directed to below-average students or schools, an inverse correlation between education inputs and test scores will result.[6] This phenomenon will obscure statistically any positive effect of randomly distributed education resources.

If its detractors reinforced the impact of the *Coleman Report*, so also did *Inequality*, the elaboration and extension of the *Report* by Christopher Jencks and his colleagues. This work

fitted the findings of the *Coleman Report* into a much broader context within which education was only one of many influences affecting income and occupational status. Not only were education inputs a weak influence on such education outcomes as years of schooling attained or scores on standardized tests, but attainment and test scores were weak influences on income and occupational status. Fully 85 percent of the variation in income, according to Jencks, was due to factors other than the education, test scores, occupation, and family background variables he was able to measure. Thus, educators could have little influence on education outcomes, a fact that might distress them but that had little instrumental significance.

Jencks' conclusion rests on a linkage of statistical results from several studies, none of which is genuinely longitudinal in character.[7] *Inequality* is therefore subject to many of the same criticisms as the *Coleman Report*, magnified by the joining together of separate survey results. Recent work by Brittain suggests that 40 to 60 percent of the variation of income, education, and occupational status may be explained by characteristics, measured or unmeasured, that are common to members of a given family (Brittain, forthcoming). Mincer (1974) estimates that from one-quarter to one-third of variations in earnings of white males is due to education when proper procedures for estimating its effects are employed. Additional work by Jencks and his colleagues also suggests that unmeasured characteristics common to family members may be quite important (see Corcoran *et al.*, 1975).

If, at the time of their publication, the *Coleman Report* and *Inequality* were the most widely known research questioning the efficacy of education, they were supported by legions of lesser known writings that had a similar cumulative impact (see Averch *et al.*, 1975, and the citations therein). On occasion, positive effects were found.[8] But although some studies showed that education inputs significantly affected education outcomes, the results were not consistent, and the cumulative impact, as summarized by a well-known evaluation of the findings of education research, was that:

> [O]verall, the input-output studies provide very little evidence that school resources, in general, have a powerful impact upon student outcomes. . . . [S]chool resources are not consistently important. . . . The results from the input-output approach do not mean that school resources fail, actually or potentially, to affect student outcomes. We simply observe that so far these studies have failed to show that school resources *do* affect student outcomes. . . . [R]esearch on teaching approaches, teacher differences, class size, and the like shows no consistent effect on student achievement, as measured by standardized cognitive tests. Work on instructional methods suggests no difference among methods; none currently appears better than conventional methods. . . . In sum, the process approach has not identified the very specific student relations involved in learning and education.
>
> (Averch *et al.*, in Levine and Bane, 1975, pp. 72-73.)

Another supporting pillar of education came under attack: The faith that education yields very high rates of return, a view that research in the early and mid-1960s by adherents to the human capital theory did so much to support, was challenged by contrary interpretations. The absence of any measure of intelligence from the earliest of these studies was an obvious possible source of bias (see, for example, Becker, 1975, and Hanoch, 1967). When subsequent work suggested that this source of bias in the estimates of rates of return to education had been less serious than initially feared (Griliches and Mason, 1972), others pointed out that motivation was unmeasured (Weisbrod, 1972); it became clear that the returns to education could never be definitively established through

survey data because some crucial unmeasurable variable correlated with education might be responsible for the income differentials human capital analysts attributed to education.

Partly as a result of the perceived failure of education to ameliorate poverty or to alter the distribution of income, a number of economists advanced theories of the labor market within which education did not contribute in the usual sense to increased productivity, although it did affect incomes and occupational status. Some argued that education was merely a screening device by which able individuals were sorted from less able individuals (Taubman and Wales, 1974). Others held that education was an elaborate and expensive device for altering the training costs that employers have to incur when they hire new workers (Thurow, 1972, 1975). Still others, from dual labor market theorists (Doeringer and Piore, 1971) to radicals (Bowles and Gintis, 1973, 1976), held that education was a device for legitimizing the distribution of workers among jobs whose characteristics, especially the exercise of power and authority, would arouse jealousy and resentment and create tension unless placement was viewed as a just reward for merit. Under this view, education, together with such traits as sex, age, race, and class-related manners, serve to cloak in the legitimizing mantle of merit the distribution of exploitive power, necessitated by capitalist modes of production. Those who held this view attacked and narrowed the intellectual support for the human capital view of education. And despite powerful counterattacks (most notably Mincer, 1974, and Welch, 1975), laymen were confused about which academic research view to follow. Since most laymen and not a few analysts choose those economic theories or interpretations that accord best with their political and social views, the shift in the popularity of various rather abstract theories hinged on shifts in the political frustrations and moods described above.

With all roads apparently blocked, which way should educators turn? There are two obvious courses. The first is to maintain that the faith in the efficacy of education should not have been abandoned in the first place—educators should challenge the research that impugns education's effectiveness, and maintain that the vigorous use of education and training to redress unequal family backgrounds or the consequences of discrimination, like Shaw's Christianity, is a marvelous idea that hasn't been tried. Such a course has much to be said for it from an intellectual standpoint, because the mountain of social science research is quite inconclusive in its policy implications, and because signs abound that research may be finding effects not previously detected.[9] The *Coleman Report* and *Inequality*, for example, are so seriously flawed that they contribute almost nothing to our understanding of the importance of education in a concerted program to alter life chances.

The second course is to consider policies other than those customarily employed by educators to affect education outcomes. This approach simply ignores the contention that education has no important effect on life chances, presuming either that this view is false or that there are some outcomes of education, including, but not limited to, the sheer enjoyment of the activity itself, that justify its continuation and improvement.

This second course opens up enormous vistas. A truly complete model of the elements of society and of the economy that affect and are affected by education would be staggeringly complex, far beyond the theoretical or empirical reach of existing social science. It would incorporate health, housing, child care and family relations, economic conditions, income distribution, crime, morality, religion, the mass media, and countless other institutions and policies; it would recognize the two-way relationship between many variables (e.g., health may affect education and education may affect health); and it would be an intergenerational model, because, for example, parents' education may affect their children's health, which affects the number of children they may have, which affects their children's test scores which . . . ad infinitum, both horizontally and vertically.

Moreover, the relationships among the various influences are probably not simple ones, but may be slightly nonlinear and discontinuous; a little bit of some things may be good, but a lot may be bad (or vice versa); a little of x, y, *or* z may do no good, but a little of x, y, *and* z may; and complex interactions may be present.

For this reason, any effort to identify noneducational policies that may have some impact on education outcomes is subject to the same biases that should have robbed narrow education research of its persuasiveness. All such research contains an enormous bias toward finding no statistically significant results because data are poorly measured, because models omit important variables, because mathematical forms are incorrectly specified, or because only cross sectional data are available when longitudinal surveys or experiments are required. When one is looking at the possible impact of, say, teacher verbal scores on student performance, the problem is serious enough. When one is looking for the impact of, say, housing policies to promote economic or racial integration on test scores, the problems are multiplied, because the causal chain through which the policy may operate contains more stages and more possible channels, each of which is likely to be relatively weak.

Nevertheless, unless educators ignore the mood that surrounds them or succumb to it, they must determine whether policies normally considered outside education may contribute something to the goals they seek. Perhaps money or lobbying devoted to such policies may improve school performance more than if they were devoted to traditional educational policies. In sections II through IV, this paper examines three kinds of policies collateral to education that may have some bearing on educational performance—policies to (a) improve health and nutrition, (b) eradicate income poverty, and (c) improve housing or promote economic or racial integration—and how they affect school performance.

It is important to keep in mind what this paper is not about. It is not about the impact of educational attainment or achievement on income distribution or occupational status. It is not about the numerous other collateral policies that might affect educational performance, such as early child care or family intervention or training. The omission of these and other steps to improve educational performance implies nothing about their effectiveness, but rather reflects the author's idiosyncracies. Finally, the paper is not comprehensive, even in the three areas it purports to survey.

To drain away whatever suspense this paper may hold, it seems clear that social science provides no justification for advocating policies in any of these three areas because of favorable impacts on education. Only steps to improve health care and childhood nutrition seem to have any clear and significant impact on educational performance, and, within the ranges encountered in the United States, these effects are not likely to be great. Furthermore, research on the educational impact of explicit policies to improve nutrition or health care is completely lacking, so that the observed positive relationship between health and education may be as spurious as, for example, radicals claim the relationship between education and income is. I want to emphasize again that results in these areas, as in education research, are likely to be biased toward insignificance. Furthermore, the confidence that can be placed in them is minimal. Therefore, the most important question policy makers must address is *not* how to extract the "truth" from the research social scientists have provided, but rather how to formulate policy with the little these scientists have given them to go on. Section V of this paper suggests how policy makers should use analysis in the formation of policy.

Though experts on medicine and nutrition have done considerable research on the effects of malnutrition, illness, and brain size on the development of infants, few have attempted to evaluate medical or nutritional policies in terms of their impacts on education. The major problem in discovering the potential for health care or nutrition affecting education outcomes is that undesirable conditions of various kinds are correlated with one another, making it difficult to determine which undesirable condition is the cause of a bad outcome.

The poor generally are less healthy than the middle class, and sustain more frequent premature births and malnutrition. Prematurity has been shown to be correlated with psychological impairment. Animal studies have documented that malnutrition, especially if extreme, results in stunted growth and reduced brain size, and that these conditions may be irremediable. Malnutrition has been found to retard learning among humans. I.Q. may depend on diet before or immediately after birth. (On all of these matters, see Douglas, 1975, Birch and Gussow, 1970, White *et al.*, 1973, National Academy of Sciences, 1974, Dayton, 1974, Barnes, 1974, Read, 1974, Eichenwald and Fry, 1974, Epstein and Weisbrod, 1974, Cravioto and De Licardie, 1974, and Winick, 1970). Furthermore, alcoholism is highly correlated with below-normal brain size and mental retardation (Jones *et al.*, 1974, and Green, 1974).

While all of these health influences on subsequent physical and mental development have been amply documented, the payoffs in improved school performance or intellectual development through nutrition programs or better health care remain very problematic. First, as noted earlier, misfortune is hydra-headed and many observers have noted the difficulty of identifying which of the many aspects of a bad environment are responsible for poor educational performance (Hurley, 1969, National Academy of Sciences, 1974, Birch and Gussow, 1970, Barnes, 1974). When poor diet, alcoholic, nonverbal, or abusive adults, and a generally desolate environment all afflict the young, it may be difficult to assign responsibility for low test scores. This kind of observation has led many to argue that correction of inadequate diet or improvement of health care alone will have little impact on educational performance, although by implication a wholesale change in environmental conditions might have significant affects (Birch and Gussow, 1970).

A second problem, in the case of malnutrition, is that the most serious consequences may flow only from egregious deprivation on a scale not widely found in the United States. Barnes (1974) observes that severe protein or calorie deficiencies associated with kwashiorkor are found principally in poor countries and that deprivation found in the United States typically is less severe. Read (1974), however, points out that subnutrition, a less severe form of inadequate diet than is common in underdeveloped countries, frequently manifests itself as anemia and that "anemia is associated with a marked decrease in attentiveness, less complex or purposeful activity, a narrower attention span, decreased persistence and a decrease in voluntary activity. As with other forms of malnutrition, the effects of anemia are related to its severity." Read continues, "Children less severely malnourished are listless, apathetic, and disinterested in their environment; such behaviors would, in turn, adversely affect cognitive development apart from any nutritional insult to brain structures." (See also Eichenwald and Fry, 1974.) Not only are the effects of borderline malnutrition less clear than are those of serious malnutrition, the extent of malnutrition is open to doubt. The immense growth of food stamps, especially their recent availability in remote rural areas, has drastically reduced evidence of malnutrition (Davis and Marshall, forthcoming).

Third, at least one study casts doubt on the seemingly unassailable proposition that healthy children will do significantly better in school than those who are not (Epstein and Weisbrod, 1974).

In a study of the relationship between school performance and parasitic diseases in St. Lucia, no significant difference was found in the performance of children with or without parasitic diseases, even after residence, sources of drinking and bathing water, knowledge of diseases, and socioeconomic status had been controlled. A severe parasitic disease, bilharzia, especially if combined with other parasitic diseases, did lead to lowered class rank and increased absenteeism, although the differences generally were not statistically significant. Although Epstein and Weisbrod noted that the special characteristics of an agrarian economy might have led to biased results—healthy children may be kept out of school differentially to perform chores and to do work, and for that reason may tend to have increased absenteeism and reduced performance—they end with a cautionary warning against drawing sweeping conclusions about the contribution of improved health to school performance.

One study (Kaplan, Lave, and Leinhardt, 1972) reports the results of a natural experiment in which similar populations were provided with significantly different health care. Children of families residing in one of two similar housing projects in Pittsburgh were made eligible for health care services in a pediatric clinic in 1968; the children in the other complex received no such service. Using regression analysis, and collecting data only on school attendance, not performance, the authors found that access to pediatric health services slightly reduced absences, by about three days per school year on the average (for whites and nonwhites, males and females). Results among subgroups were inconsistent, perhaps, the authors suggest, because of small sample size. On balance, the authors conclude, "A comprehensive care project does produce an improvement in attendance, but the effect is small compared to other variables, such as the proximity of the school, race, sex, and socioeconomic conditions." As this summary makes clear, if health care is viewed as an instrument for improving school attendance, its costs must be weighed against other apparently unrelated outlays, for example, smaller and less separated schools will reduce average distance from schools and, according to Kaplan, Lave, and Leinhardt, this will improve attendance. The fancifulness of such an exercise in program analysis—the comparison of the costs and benefits of improved health care and of smaller schools for improved educational performance—makes clear the resistance to viewing health care in such terms.

Two other considerations dictate caution in discounting the potential of improved health care to affect educational performance. The first is that much of the research just cited is cross sectional. The discovery of possible biases in cross sectional research on education inputs should alert the consumer of similarly structured health research to the possible presence of similar biases. The second is that sample sizes in medical research are frequently small and data to control socioeconomic characteristics are often lacking.

The foregoing survey offers little support for the idea that improved health care or nutrition should be supported *because* of its beneficent effects on school attendance or improved performance. At present the case for such programs must stand or fall on other grounds. I found no research on the effects of health care programs on school performance tied to the school. Much the same can be said for increases in the extent of nutrition programs. With the explosive growth of food stamps, the extent of serious malnutrition has declined rapidly, making it unclear whether even costly increments to nutrition programs would have sizeable effects on nutritional adequacy, how extensive the remaining nutritional inadequacy is, and the extent to which such remaining nutritional inadequacy would respond to expanded nutrition programs. It is possible that nutrition programs focused on very poor children, particularly if managed by the schools, would yield some benefits in terms of school performance, but the evidence is lacking; it is hard to believe that one's support for such aid would hinge on whether or not attendance was increased a few days or if test scores rose a few points.

III

That income and education are correlated is a universally recognized fact. High incomes and ample education go together, not in lock-step to be sure, but generally speaking. The nature of this correlation has been the subject of hundreds of research papers and books by economists, sociologists, and others. Most frequently, these researchers have been interested in how much an increase in education would affect income or occupational status. In many cases, analysts have tried to take into account more complex relationships in this area, for example, that parental income affects the amount of education their children will receive, which then will affect the children's income.

The key point in such research is that causation runs both ways: from education to income and from income to education. Most studies have found that higher parental income has some positive impact on educational attainment of children, even after such other background factors as occupational status, parental education, race, and intelligence have been statistically controlled (see, among a vast literature, Jencks, 1972, Bowles and Gintis, 1972, 1973). Such findings lead naturally to the supposition that the provision of income to low-income households may have some impact on the education of children in such households. In the absence of careful experiments, however, one cannot exclude the possibility that the observed relationship between parental income and the education of their children may be due to some unmeasured factor, such as parental drive, and that additional income would have no impact on educational performance whatsoever.

Studies based on data from the two income maintenance experiments thus far completed have attempted to discover whether giving money to low-income families improved the educational performance of their children (Maller, 1974, Maynard, 1975, and Maynard and Crawford, undated). On theoretical grounds, additional income may be expected to improve educational performance for several distinct reasons. First, additional income may lead to the purchase of certain kinds of commodities shown in previous studies to be correlated positively with educational performance. Second, cash assistance may reduce the family's need for part-time work by students and increase their time for school work. Third, to the extent that cash assistance leads to reduced labor supply by adults, to the extent that additional nonwork time is spent with children, and to the extent that such time helps the child's school performance, education may be advanced. Possibly offsetting these effects, a rise in income, together with a reduction in work, may reduce increases in consumption of goods and in the use of time for purposes that do not advance or that conflict with education. It is even conceivable that such diversion of time would reduce school attendance or time spent in the home in learning activities. In plain English, the additional income may go for books or booze or color TVs, and the TVs may be turned to *Nova* or *S.W.A.T.* (To make matters worse, we are unsure what difference these alternatives make.) On theoretical grounds it is unclear whether a negative income tax (NIT) or some other form of cash assistance will increase or decrease educational performance; the cross sectional studies mentioned above make the former outcome seem more likely.

The first negative income tax experiment took place in New Jersey and Pennsylvania. Participants were divided into eight groups. Members of seven groups received cash assistance under one of seven plans, members of the eighth, a control group, were paid a small fee for providing the same information obtained from other groups, but received no other assistance. The seven negative income tax plans differed according to the payment a household with no income would receive—the "income guarantee"—and according to the rate at which benefits were reduced if the household had earnings—the "tax rate." The income guarantees were 50, 75, 100, or 125 percent of the Social Security poverty threshhold. The tax rates were 30, 50, or 70 percent. Only seven of the twelve possible combinations were employed.

The experiment was plagued by a number of difficulties, perhaps the most notable of which were the drastic revision of the welfare program in New Jersey in the middle of the experiment and the difference between the Pennsylvania welfare plan and either of the two New Jersey plans. As a result, the exact size of the experimental stimulus to which households were really exposed is almost impossible to discover.[10] For this reason it would be rather surprising if any of the differences in educational performance between families eligible for NIT payments and the control families were very large. In addition, payments under the experiment were made for only three years, and the impact of income on educational performance might depend on income over a much longer period. To the extent that this is true, any statistical results probably would represent lower limits of the long-term impact of a negative income tax on educational performance. One cannot exclude the possibility that the experiment tells little or nothing about the results of a full-scale negative income tax, because such a program would change the income, consumption, and possibly the habits and values of whole neighborhoods; the impact of changes in behavior by neighbors and friends cannot be observed under an experiment that aids only isolated families.

Against this background, the fact that few statistically significant results were obtained may come as no surprise. The search for effects of the New Jersey-Pennsylvania NIT was limited to the school attendance of 16-, 17-, and 18-year-olds. Although some groups (experimental families were classified as white, black, or Spanish speaking) seemed to have increased school attendance in some of the 12 quarters of the experiment, others seemed to have decreased attendance, and none of the effects persisted throughout the experimental period. In a flight of hyperbole worthy of P.D.Q. Bach, one analyst refers to the impact of the experiment on school enrollment in the ninth quarter as "overwhelmingly statistically insignificant." The results for the other quarters seem only slightly less overwhelming.

The second income maintenance experiment took place at two widely separated rural sites, one in North Carolina and one in Iowa. The families served differed markedly between the two sites. Those in North Carolina were mostly black, with incomes normally around the poverty thresholds or below. Those in Iowa were entirely white and tended to be only temporarily in poverty. As in the New Jersey Income Maintenance Experiment, the Rural Income Maintenance Experiment employed a variety of negative income tax plans, five in all, with guarantees equal to 50, 75, or 100 percent of the poverty threshold, and tax rates equal to 30, 50, or 70 percent. Variations in the NIT plan, however, had little bearing on the analysis of how cash assistance affected school performance.

The investigators examined whether negative income tax benefits affected school absenteeism, comportment, academic grades, and standardized achievement tests. They classified children in two groups—grades 2 through 8 and grades 9 through 12. On balance, children in experimental families in North Carolina seem to have done better in school than those in control groups; in Iowa, no significant differences were detectable. But, again, the results are not consistent or dramatic.

Absenteeism declined, but by statistically significant amounts only in North Carolina for grades 2 through 8. Comportment improved by statistically significant amounts in North Carolina in all grades, but declined by insignificant amounts in Iowa. Academic grades increased in North Carolina, and decreased in Iowa, but the changes approached significance only in North Carolina for grades 2 through 8. Scores on standardized tests dropped in Iowa, but rose in North Carolina; the drops were larger in Iowa than in North Carolina, but the changes were insignificant in Iowa for both grade groups, while they were significant in North Carolina for grades 2 through 8.

Efforts were made to determine whether experimental effects depended on the negative income tax plan or on the interaction between negative income tax payment and a large number of other household characteristics, but in no case was there any consistent pattern. The analysts observe that "In both regions, the mean experimental responses tend to be accounted for by relatively large changes in the performance of a small portion of the total sample" (Maynard and Crawford, undated, p. 89), and suggest that detailed analysis of what is going on inside the home is necessary to explain why the experiment does a lot for a few, but nothing for most.

Using the same data, Maynard reported in a separate study that school performance seems to be affected by the things money buys, if not by money itself. In North Carolina, absenteeism is correlated negatively with housing space per individual (particularly for grades 9 through 12), time spent with children by the mother (measured by the difference between a normal work year and the time the mother spent in the labor force), a large number of home environmental characteristics, and some school characteristics. Once again, some variables that were significant for younger children were not significant for older children, and the differences were not always intuitively plausible.[11] Some variables that had no apparent effect on absenteeism included nutritional intake and presence of high-quality newspapers, magazines, or TV. A similar pattern of instability of results appears in the impact of the different variables on the other indices of school performance.

What should one make of these results? First, the discovery of any significant effects must rank as something of a surprise. The three-year duration of the experiment is a more serious limitation in discovering the long-run effects of income maintenance on educational performance than it is for determining impacts on labor supply. The channels through which additional income may affect educational performance are not only problematic but indirect, and are mediated by habits and values that income may alter only gradually, if at all. The fact that the most significant results seem to be associated with absenteeism, rather than with comportment, grades, or scores on achievement tests, is a bit reassuring. Although the rationalization is *post hoc*, one might well expect that the first effects of higher income would be on attendance, and only after a lengthy period, perhaps a good deal more than three years, would one observe changes in comportment, grades, or standardized tests (weakly significant positive effects on test scores of both blacks and whites were observed in North Carolina). If one is willing to make rather simplistic use of some of the coefficients derived from the experiment, one can argue that the long-run results of the experiment will be at least twice as large as those observed over the period of the experiment.[12]

The striking contrast between the heavy sprinkling of significant results from North Carolina and the absence of such results from Iowa is a major puzzle. The answer may be that income in the amounts provided through the experiment matters more for recipients whose poverty is endemic (such as those in North Carolina) than it does for recipients not normally poor (such as those in Iowa). It may be that other unmeasured cultural dissimilarities between families in the two states or other environmental characteristics of the two sites account for the differences. But, whatever the reason, the fact that "nothing matters" in Iowa means that income, like education inputs, does not yield "consistent" results and must give pause to any quick or facile advocacy of income maintenance as an educational strategy. Even the effects observed in North Carolina are small enough to cast doubt on the *efficiency* of income maintenance as an educational strategy.

The results of these experiments may be altered by research now under way on additional data gathered in two other income maintenance experiments (Gary, Indiana, and Seattle/Denver). They are quite consistent with one recent cross sectional study that concludes that attempts to reduce the disparity in the probability of college attendance by increasing income transfers to those of lower socioeconomic status will meet with little success (Christensen, Melder, and Weisbrod, 1975).

IV

As long as the neighborhood school is something people care about, and as long as people think that who their children go to school with is important, government policies about the composition of neighborhoods must be regarded as educational policies. In practice, neighborhoods are influenced directly by housing policies of all kinds and by zoning regulations and processes, and indirectly by a wide range of government actions concerning highways, parks, and police and fire protection—in short, by almost everything that governments do.

To keep the subject within comprehension, if not within bounds, I shall consider only policies that affect housing quality or the racial or economic composition of the neighborhoods.[13] Such policies include housing subsidies or zoning to reduce the economic or racial homogeneity of neighborhoods. While they do not include policies to promote school integration through any of the numerous possible techniques, the bulk of the evidence on the impact of changes in neighborhood composition must be inferred from studies of school integration.

Housing Quality

For nearly a century, housing reformers have claimed that a wide variety of problems—bad health, crime, family dissolution, poor school performance—are attributable to poor housing. Although early studies were not careful to make clear whether they were talking about the deleterious consequences of living in bad structures or in bad neighborhoods, later studies did make this distinction clear. The classic, but far from definitive, work (Wilner *et al.*, 1962; also see Wilner and Walkley, 1963) addressed the question of how housing quality, as opposed to neighborhood quality, affects school performance. The conclusion was that housing *per se* has no impact on test scores, but may have a small effect on attendance and, via attendance, on promotion.

This study was based on a sample of mostly black applicants for admission to a new public housing project in Baltimore. Before the project was opened, each household in the sample was paired with control families chosen on the basis of a large number of variables. Before and after admission to the projects, students were given a battery of tests—the Kuhlmann-Anderson or the Otis IQ tests, an arithmetic test, and a reading test—promotions were counted, and attendance was measured. It is not clear what the reading and arithmetic results mean, however, since the additional children who entered the school-age population during the study were included in the "after" tests, but not in the "before" tests. Children in the housing project attended school about eight more days per year than did the children from the control families, a statistically significant difference. As in the Rural Income Maintenance Experiment, attendance seems to be the school output most immediately affected. A small but statistically significant difference in promotion distinguished children in the project from controls.

Because the Wilner study is simply a comparison of group means, however, the results may be stronger or weaker than the authors suppose. For example, Kaplan, Lave, and Leinhardt (1972) found that the distance of a housing project from a school is a significant influence on attendance. No mention of this factor or of the many others that might be relevant influences on attendance appears in the Wilner study. Of perhaps greater importance is the fact that moving into a housing project changes one's neighbors. If peer group pressures and standards materially affect educational performance, the impact of movement into improved housing with different neighbors may be considerably larger or smaller than that detected.

The Rural Income Maintenance Experiment provides some support for the proposition that additional space has a positive impact on school performance of children from very poor families, especially those in high school. On balance, however, evidence simply does not exist that would justify housing subsidies on the ground that they will lead to major improvements in educational performance. Improved housing may lead to some improvements in attendance and after many years these may lead to small improvements in promotions, but programs to improve housing quality, like those to improve health or nutrition, will have to stand on their own.

Neighborhoods and School Composition

A more interesting and much more divisive issue than housing quality concerns the impact of economic, racial, or class integration on educational performance. At the most basic level, every parent "knows" that his childrens' development is influenced profoundly by their friends and playmates; that a school environment is shaped by the attitudes and behavior of the children who attend it; that children are happier or learn more in some kinds of classrooms than in others; and that classroom atmosphere depends particularly on the behavior of the children in it. Because he knows these things, every parent also knows that the kind of neighborhood in which his children grow up will influence how his children grow up and how much they learn in school. Every parent has known these things for a long, long time.

Social scientists, however, have "known" these things for only a few years, and, in the sense in which social scientists really "know" things, it is questionable whether they know them even now.

The study of determinants of educational performance did not begin with the *Coleman Report*; that study, however, carried so much more weight and enjoyed so much more attention because of its official background than work that had gone before it that it has become the point from which all subsequent discussions of the impact of neighborhood conditions—the aspects of housing other than structural quality—begin. According to the *Coleman Report*, the characteristics of the student body have a greater impact on school performance than do school facilities, school curriculum, or teacher characteristics, especially for minority children (defined as blacks, Puerto Ricans, and Mexican-Americans, and not including orientals, whose survey characteristics closely resembled those of whites). Reanalysis of data from the Equality of Educational Opportunity Survey (EEOS) indicates that this conclusion was exaggerated and that the influence of peers on test scores could not be measured with the EEOS data (Smith, in Mosteller and Moynihan, 1972). The racial composition of the school appeared not to have a material impact on test performance after the students' own background, school facilities, curriculum, and student body characteristics other than race had been controlled. Actually, the *Report* was rather unclear about the impact of race; one part of the *Report* was interpreted as suggesting that racial composition of schools had an independent effect on school performance, so that blacks would benefit and whites would be unaffected by movement from segregated to integrated schools, even if the schools and student bodies were identical in all other measured respects. Subsequent analyses (Bowles and Levin, 1968, Jencks, 1972) indicated that this was not so. And some parts of the *Report* itself may be read as saying that race, *per se*, makes no difference on school performance.

There is serious doubt about how much weight can be attached to the conclusions of the *Coleman Report* regarding the impact of the student body on the performance of other students, however well this aspect of the results coincides with "what every parent knows." First, the statistical analysis on which the *Report* was based contained an error that overstated the impact of

peers (Smith, 1972). Second, as Bowles and Levin (1968) first pointed out, and Smith confirmed, the survey contained grossly inadequate controls for the students' own background. Because families of similar backgrounds tend to cluster, the characteristics of neighbors and schoolmates may simply be a better indication of a family's own characteristics than are the values of those characteristics reported in the survey. (If a high school dropout mother and her children reside in a neighborhood of BA's, the neighborhood education level may tell more about the mother than does her own degree.) If this criticism is valid, then the statistical impact of the student body is smaller, perhaps much smaller, than the statistical impact reported by Coleman. (Furthermore, the results of the *Coleman Report* may well be biased toward overstating the importance of neighborhood characteristics; see footnote 5.)

If student body characteristics are presumed to affect educational performance, the question arises of how pupils with different characteristics should be distributed in order to maximize their performance. Posing the question in this way pushes to the side other considerations influencing school composition (such as the constitutional prohibition of racial segregation and issues of interpersonal equity) and leaves for later discussion how any particular composition should be achieved. One reanalysis of data used in the *Coleman Report* reached rather strong conclusions about the "best" racial composition of schools. David Armor (1972) found that test scores of both whites and blacks are highest when 1 to 25 percent of the school population is black. Unfortunately, this conclusion is based on naive procedures (simple plots based on racial composition, without controlling for other variables) and suffers from all of the shortcomings that inhere in the underlying data.

Two recent studies have suggested that educational environment may affect different students in very different ways and that the effects are cumulative. Summers and Wolfe (1975) analyzed data on students in Philadelphia public schools and found that both black and white elementary school pupils do best when schools are about half black and half white; that in junior high blacks do better and whites do much worse when the school is predominantly black; and that both blacks and whites do better in integrated than in predominantly white junior high schools. The effects of having ability levels mixed are even more variable. In elementary school, having high achievers in a class helps low achievers, and the presence of low achievers does not hurt high achievers. At the junior high level, average students do not perform less well when below-average students are present, but all students score better when high achievers are present. Thus, spreading around high achievers reduces the scores of some students and raises them for others. Summers and Wolfe also found that certain school or teacher characteristics help some students and hurt others. If this result is generally valid, it would help to explain why particular inputs show up as quite effective in some studies but not in others (Averch *et al.*, 1975).

Henderson, Mieszkowski, and Saubegeau (undated) report that the average I.Q. of fellow students has a highly nonlinear effect on I.Q. test scores. For a sample of Canadian pupils in kindergarten and the first three primary grades they find (a) that being in a very poor class (defined as students in the bottom fifth on I.Q. tests) has a deleterious effect on I.Q. growth, but that being in the third, fourth, or fifth quintile is only slightly more advantageous than being in the second; (b) that the importance of classmates' I.Q. is more important in French, a subject requiring pupil interaction, than in mathematics, a subject one learns more on one's own; (c) that the long-term cumulative effect of being repeatedly in a high I.Q. class is roughly two and one half to three times as great as the impact of a single year.[14] Henderson *et al.* also found that the effect of variation in the range of abilities found in the classroom differed over time and between subjects. High variance was beneficial and low variance harmful in French, but the effects became smaller in the second and third grades than in kindergarten and first grade. High variance was beneficial in mathematics in kindergarten and first grade, but was harmful in second and third grade, as was low variance in all grades.

In short, I.Q.-integrated classrooms during the first four years of primary school seem to be more conducive to improving I.Q.'s than are homogeneous classrooms, but extremely heterogeneous classes may make teaching more difficult in some subjects and especially for older primary pupils. Unfortunately, Henderson *et al.* do not have data on older pupils. The heterogeneity of results found in the first four years makes it foolhardy to extrapolate these results to older pupils. As the level of instruction advances, relatively homogeneous classes might become more or less important, and the impact of being in the presence of especially bright classmates might become more significant for some subjects or for some kinds of pupils.

The Summers-Wolfe and Henderson *et al.* studies lend some support to the idea that schools integrated as to abilities of pupils help, rather than hinder, learning. However, this effect may be confined to the early years, and neither study says anything about how such ability integration is achieved. In both studies, no effort was made to mix students or to segregate them for other than the customary purposes of the school systems involved. Only the Henderson *et al.* study is explicitly longitudinal. Summers and Wolfe also suggest that racial integration is not harmful, and may be beneficial. Neither study says anything about economic integration. The dominant result of the Summers-Wolfe and the Henderson *et al.* studies is that analysis based on simple linear models applied to cross sectional data, the kind collected in most surveys, may be essentially worthless.

These studies do not directly tie housing or neighborhood policies to educational performance. They do bear on the effects of such policies on educational performance to the extent that those effects operate through changes in the intellectual or racial composition of classes. But such policies could operate in other ways, for example, on pupil attitudes or on habits acquired outside the classroom. Effects felt through such alternative channels might reinforce or cancel out those observed by Henderson and his colleagues.

While natural experiments that show what really happens when public policy requires economic or intellectual integration in the classroom do not exist, court decisions over the past two decades have generated ample data on the consequences of racial integration on classroom performance. Unfortunately, little data have been collected, and most of what has been written about data actually gathered has been based on inadequate or incomplete models. In a recent survey, St. John (1975) asserts that the findings from these studies have been inconclusive. Longitudinal studies do not conclusively support the proposition that desegregation improves achievement. The effects seem to differ across subjects; in contrast to Henderson *et al.*, St. John reports that the most immediate improvements seem to come in mathematics, with any improvement in verbal skills coming only after a lag. Most studies ignore the impact of desegregation on white students. The impact of desegregation on self-confidence is equally ambiguous. Many studies have looked for simple, prompt changes in measures of self-esteem, but these have not been observed. The striking fact is that measured self-esteem of blacks is higher than that of whites at corresponding achievement levels (Epps, 1975) and exposure to whites seems as likely to lower self-esteem as to raise it.

Apparently, the impact of desegregation in this area, as well as on achievement, depends on whether integration occurs only in the school as a whole, or in the specific classroom as well; on community attitudes toward desegregation; and on the manner by which integration is achieved.

Results of studies about the impact of integration on racial prejudice are also mixed and contradictory. The studies are hindered by lack of adequate control variables, for example, for socioeconomic status, sex, preintegration achievement levels, and for the way in which desegregation occurred. The biggest decline in racial prejudice seems to occur among young whites and older blacks. Positive findings seem to be more numerous among recent studies.

Another evaluation of the impact of studies of school desegregation (Weinberg, 1975) concludes that (a) much of the evidence of positive effects on achievement is questionable and inconsistent, but (b) no valid evidence suggests that it is harmful. Of the dozen studies, only two show negative effects, and these use grades rather than achievement scores. Cohen (1975) and St. John (1975) are more agnostic. Cohen concludes that evidence is so mixed that any position can be supported or refuted by studies now available to litigants in integration cases.[15]

The implications of these studies for policies to alter neighborhood composition are far from clear for several reasons. The first is the ambiguity of the results of the studies themselves. Second, it is not obvious that the studies would be negative even if they uniformly supported a particular effect on racial, economic, or other kinds of integration or segregation. Housing policies have ramifications far beyond education. As Jencks (1972, p. 106) put it:

> The case for or against desegregation should not be argued in terms of academic achievement. If we want a segregated society, we should have desegregated schools. . . . The effects of segregation on test scores are certainly not large enough to justify overriding the preferences of parents and students.

Nothing has surfaced since Jencks wrote these words to reverse his conclusion. Third, at least as far as racial integration is concerned, the issue in the first instance is one of constitutional imperative, not social scientific findings. One respected jurist has suggested that the role of social science in the original Brown case and in subsequent litigation on segregation has been drastically overrated (Wisdom, 1975), and another has pointed out that just when social scientists disagree, as they increasingly have done, a jurist, not trained to evaluate the conflicting findings, is driven to ignore all of them; cynics might suggest that the same should be true for educators (J. Skelly Wright, quoted by Levin, 1975).

V

The preceding three sections have reviewed some of the evidence on the impact of three kinds of non-educational policies on education outcomes. In every case the results are equivocal—the policies may have some effect on education outcomes, but the evidence is too flawed to rely upon, the results of several studies are inconsistent, or the indicated effects are small. What inferences should educators draw from these results about their positions, as educators or as citizens, on such non-educational policies?

Two responses are unjustified. First, the lack of conclusive evidence means that educators cannot use social science to support one or another position on health care, income redistribution, or housing or neighborhood policies on the basis of improvements in education outcomes traceable to such policies. The evidence just will not support them; or, to be more precise, the evidence will support both them and those arguing the opposite position. Second, and perhaps less obviously, educators cannot conclude that the conflicting evidence means that these policies have no significant impact on education outcomes and that, hence, educators should stick to their traditional concerns—school resources, organization, curricula, and financing. The quality of studies, for the most part, is bad enough—the evidence is questionable, key variables are omitted from the data, the models are naive or at least do not include many plausible forms—the hypothesis that these policies *do* affect education outcomes cannot be rejected. Donald Campbell (1974) has observed that in social science almost all theories are "radically underjustified"; in other words, many plausible theories other than those that are currently accepted frequently can explain a particular set of facts as well as do those that we accept. If this is true, it also is the case that our acceptance of the null hypothesis—that policies have no effect—is also unjustified. In other words, we accept one hypothesis and reject others for reasons unrelated to social science.

All of the studies cited in this paper are partial analyses. In every case, a single aspect of the environment is altered. In none are several or all modifiable aspects of the environment changed. Yet it is quite possible that improvements in education outcomes depend on the simultaneous satisfaction of several necessary conditions, no one of which is sufficient. A wide range of analysts agree that the total environment has a major impact on education outcomes (Jencks [1972] suggests that 25 to 40 percent of test score inequality is due to environment; Brittain [forthcoming] holds that a sizeable proportion of differences in education attainment is due to family-related variables), yet no single alteration of that environment seems to have a major impact. Writers on the relationship between health and education seem particularly aware of this possibility. In short, the educator may have to take a position on a wide range of non-educational policies if he is to take a stand on any, but without really firm evidence as to which ones matter.

Although policy makers seldom articulate how they use social science in reaching decisions and are seldom asked to do so (Caplan, 1975), I suggest that they generally choose one of three courses in using research in the formation of policy: "least risk," "best bet," and "winning line." Each is appropriate in some circumstances. I will argue that the "winning line" approach is indicated for educators in their approach to non-educational policies.

In general, a number of alternative social science theories exist about the relationships among any given set of variables. Consider a decision about whether to liberalize income maintenance through some kind of negative income tax, a children's allowance, or some other scheme. To a degree this decision may hinge on estimates of the impact of each scheme on labor supply. One's predictions about the impact of such policies on labor supply will depend on which of the various estimates of the labor supply behavior of low-income households one accepts. According to the

"least risk" strategy, the policy maker would test each plan against each estimate of labor supply behavior. For each plan, the worst outcome would be noted, and that plan would be chosen for which the "worst results" are least unfavorable.

This strategy, referred to as "minimax" (minimize maximum regret) in game theory, commonly is used in defense planning. Weapons systems are evaluated with various possible events in mind, and are arrayed from best to worst according to the severity of the worst set of events should each be adopted. When matters of life and death are involved, and particularly when the adversary can be regarded as malign and intelligent, this approach to decision making has considerable appeal. When the adversary is not purposeful (for example, when the adversary is random chance), and when something less transcendent than survival is involved, it becomes desirable to consider outcomes other than the worst that a particular choice can lead to.

The policy maker attempting to decide whether to increase education inputs may well react to the plethora of estimates of their impact by selecting the one estimate he considers best (the "best bet") and then acting as if that estimate were known with certainty to be correct. The "best bet" strategy is the common response of the policy maker to a complex world in which choices are numerous and his only adversary is an indifferent nature. According to this strategy, one considers the alternative theories about how some outcome is determined. One estimates the probability that each of the theories is correct. The theory with the highest probability is accepted. Then policies are formulated *as if that theory were known to be true with certainty*. The fact that the theory is not known with certainty and that other theories may be correct is ignored; so is the possibility that if other theories are correct, some other policy may be best. The complexity of the world and the difficulty of simultaneously considering a wide range of alternative theories and what happens if each is correct leads to the exclusion of much information.

What is less obvious is that the "best bet" approach suppresses value judgments. Even if some outcomes are considered much more important than others, the "best bet" approach may cause them to be ignored. This happens if the desired outcomes can be achieved only, or more easily, if some theory is correct other than the one that leads in the probability race. Suppose, for example, that one attaches very great weight to the equalization of test scores. Suppose also that the theory of education most in accord with available evidence suggests no policy will have a significant impact on test scores. The "best bet" strategy would be to do nothing. But it seems clear that one might choose to increase education inputs anyway, because doing so is the *only* course that gives any promise of succeeding. In another context, the "best bet" strategy in war is always rejected by the losing side until the last day. Once eventual defeat becomes more probable than victory, surrender is the "best bet" course. The fact that losing sides fight on suggests that some other criterion is being used.

In war and in games of chance, the participants typically follow a "winning line" strategy. They have more or less well-defined goals and formulate policy by asking in what states of the world those goals can be achieved. They then proceed *as if* those states of the world existed. Only if that assumption is indisputably absurd or if costs become excessive is "surrender" indicated. In the terminology of statistics, the participants maximize the "expected value" of the final outcome.[16]

The "winning line" strategy is the correct approach to the formulation of social or economic policy as well. In evaluating a particular course of action, one must consider its consequences under various possible states of the world, not only the one state that seems most probable. Today's faddish theory may turn out to be false and the chance that some other theory may be correct, even one now regarded as passé, should not be ignored (Packenham, forthcoming). One can never

bloodlessly base policy on analysis. Values, passion, and devotion enter at the very outset, as well as simultaneously with the findings of social science.

Consider the issue of how much genetic inheritance affects educational achievement. Theories differ widely. The lay policy maker must choose among expert opinions he is not equipped to evaluate. He knows that matters of great importance may hinge on how much of the variation in observed ability is due to inherited and immutable factors and how much is due to alterable environment. The "best bet" strategy would require the policy maker to choose that theory that he thinks is more likely than any other to be true, and to base policy on that perception. By contrast, the "winning line" strategy would require him to focus on the consequences that follow from the correctness of alternative theories. For example, in addressing the question of genetic inheritance versus environmental influences, specifically school influences, on achievement, Douglas (1975, p. 14) wrote:

> The case for a major environmental component in children's performance on tests and in schools seems to me to be a very strong one. And both because it is only this portion of the variance which we *can* affect, and because the consequences to society of assuming that genetic differences dominate seem to me to be unacceptable, I think we must operate *as if* experience—including schooling—plays, or can play, a major role in shaping the child's intellectual competence and performance.*

In a similar vein, Cohen has asserted that social science serves the courts to the extent that research findings are pertinent to underlying value issues under court scrutiny.

Educators who follow the "best bet" strategy in deciding whether and how to take stands on non-educational policies that may influence education outcomes must stand aloof or oppose the use of resources in such areas. The available evidence is just not sufficient to support the use of limited lobbying effort or the possible diversion of limited budget resources from direct education outlays. But educators cannot really adopt this position and stay in business. To do so honestly would require also that they cease fighting for education resources. For, as Averch *et al.* (1975) have concluded, available social science research, flawed though it is, does not support an increase, and may not even justify maintenance, of such resources. As far as education resources go, educators must take the "winning line" approach; they must act *as if* such resources were useful, and they can do so because the quality of most existing research is so poor, and because the issues are so very complex, and, most of all, because of their devotion to certain goals. They can perhaps take comfort from a paraphrase of Hume: No evidence is sufficient to establish an implausible result unless the unreliability of the evidence would be more remarkable than the result which it endeavors to establish.[17] Given the shoddiness of much that passes as education research, this test is not a difficult one to pass.

But if the "winning line" approach is the correct one for educators to take toward educational policy, it is also the correct one to take toward non-educational policies. Educators know less in these areas, but they do know some things. The effects on education are likely to be indirect and possibly smaller than those of purely educational policies. Whether their voices will carry less weight in the lobbying process on non-educational policies because they are not experts, or more weight because they are viewed as detached and disinterested, is unclear. Educators are unlikely to find it in their interest to devote large amounts of time or resources to health, housing, or income

*Emphasis in original.

redistribution. But the "winning line" approach requires that they devote some. The chances that the egalitarian objectives of educators (they have many others, of course) can be achieved without changing the status quo in these areas seems very slim. If educators care about egalitarian objectives, however, they must heed the words of the philosopher: "If I listen to the irony slinking behind all things, it slowly emerges, blinking its small clear eye: live as if . . ." (Brée, 1964).

FOOTNOTES

1. A recent survey of the concerns of the American public found that interest in "improving our educational system" ranked seventh in a list of 31. *See* Watts and Free (1976).

2. Completion rates refer to 25- to 29-year-olds. Black completion rates are continuing to rise while white completion rates have levelled off.

3. Freeman's conclusions are challenged, however, by unpublished tabulations prepared by Denison (1976).

4. Wilson suggests that the urban crisis inheres in a decline of standards of interpersonal behavior. "This issue which concerned more respondents than any other was variously stated—crime, violence, rebellious youth, racial tension, public immorality, delinquency. However stated, the common theme seemed to be a concern for proper behavior in public places" (p. 26).

5. Luecke and McGinn created a simple model of the educational process including promotions, pupil mobility, and the *assumed* positive impact of various education inputs, including teacher characteristics and per pupil expenditures. This model is simulated—that is, pupils proceed up the education ladder. At a certain point this dynamic process is interrupted and data on a cross section of students are "collected." The kinds of analyses contained in the *Coleman Report* are then performed. Based on the results of these analyses, Luecke and McGinn conclude, "Our results are consistent with the position that cross sectional data can tell us little about how a dynamic system has worked in the past, or how it could be made to work in the future. The more complex and more dynamic the system, the less likely are conventional analyses to yield reliable inferences about it. . . . [I]f independent variables change over time in relation to others, the direct effects on the dependent variable are likely to be underestimated. In this study the results tended to exaggerate the direct effect of Family on Achievement, because stability of Family resulted in a relatively high final-period correlation with Achievement. The direct effect of Teacher on Achievement was usually underestimated, using contribution to variance as the indicator, since Teacher changes most frequently from period to period. Adding complexity would have resulted in ever greater misrepresentations of the cynamic variables, since in multiple regression the more independent variables one adds, the less variance each new variable is likely to explain. . . .

   "[O]ur results suggest that studies which find little or no relationship between educational inputs and achievement may be highly misleading. Our findings suggest that the combination of data and statistical technique most often used is unlikely to reveal such relationships even when they exist."

   In plain English, this study suggests that little weight should be accorded the conclusions of the *Coleman Report*, and that the debate about it did little to advance our understanding of how education affects children, except insofar as that debate led to the conclusion that the method of analysis used in the *Coleman Report* is fatally inadequate.

6. This problem of endogenous policy instruments has been recognized by macroeconomists attempting to sort out the relative power of fiscal and monetary policies with historical data. Alan Blinder and Stephen Goldfeld (1972) have shown that if both fiscal and monetary policy are used to offset forecast swings in economic activity, estimates of their effectiveness will be smaller than their actual effectiveness, and the effectiveness of the instrument based on the

better forecasts will be understated more seriously. The downward bias in estimates of the effectiveness of education inputs occurs only to the extent that differences in inputs are associated with students or schools above or below average after controlling for all included variables.

7. *Inequality* relied in part on the Plowden Report and Project TALENT, which collected follow-up information from students on whom data were collected concerning education inputs at one point in time. Neither report, however, contained data on education inputs over several periods and, hence, were cross sectional in their most essential element. *See* Peaker, 1967, Flanagan *et al.*, 1962, Flanagan *et al.*, 1971, and Shaycoft, 1971.

8. *See*, for example, Anderson (1976).

9. Research has accumulated suggesting that a curriculum affects what children learn, if not how much (Walker and Schaffarzick, 1974); that "Follow Through" works (Anderson, 1976); that peer-group tutoring is effective (Bloom, 1976); and that other educational procedures matter. There is some evidence that differences among schools can move students 10 to 20 points up or down in percentile rankings on test scores (Klitgaard and Hall, 1975, Jencks and Brown, 1975).

10. The stimulus is defined as the difference between the NIT payment and the welfare payment a household was eligible to receive. *See* Aaron, in Pechman and Timpane, 1975.

11. For example, the score of the mother or female guardian on the Ammons Quick Test is negatively related to absences for grades 2 through 8, but positively related for grades 9 through 12. Children of farmers had more absences than controls in grades 2 through 8, but fewer in grades 9 through 12.

12. The data include as an independent variable the pre-experimental value of the dependent variable. The coefficient of this variable tended to be around .5. If for any dependent variable, y, and any independent variable, x, $y = a + bx + cy_{-1}$, then in equilibrium $y = [a/(1 - c)] + [b/(1 - c)]x$. In this case c equals approximately .5, suggesting that $b/(1 - c)$ is approximately twice b, the coefficient for x. On the other hand, this coefficient may only express a finding that about 50 percent of deviations of pretest scores from the mean are "true" deviations and that the other half is random error in the pretest.

13. I assume neighborhood schools. If schools draw students from a wide area, neighborhood characteristics may still affect the academic performance of children through the norms, values, and habits of the children with whom they play or of the adults with whom they come in contact.

14. The long-term effects are based on the kind of analysis described in footnote 12, in which the previous year's score on a subject is one of the variables explaining the current year's score. This "long-term" interpretation of the coefficient of the lagged variable is more justified here than in the Rural Income Maintenance Study because Henderson *et al.* have explicit data for each pupil for four years, while only cross sectional data were used in the analysis of the rural experiment.

15. Henry Levin argues that this distressing circumstance exists for every issue concerning education likely to come before the courts.

16. The payoff, or expected value, E_j, of policy j is $E_j = \sum_i \sum_k p_i q_{ijk} w_k$, where p_i is the probability that theory i is correct, q_{ijk} is the contingent probability that if theory i is correct, policy j will achieve goal k; and w_k is the value of that goal, net of the costs of achieving it. The "winning line" strategy chooses the policy that makes E_j as large as possible. The "best bet" strategy identifies the largest p, say p_n, and then selects the policy that makes E_{njr} the value of the outcome, assuming theory n is correct, as large as possible.

17. *See* Goode (1966) who quotes Hume verbatim: "No testimony is sufficient to establish a Miracle, unless the Testimony be of such a Kind, that its Falshood (*sic*) would be more miraculous, than the Fact, which it endeavors to establish. . . ."

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